Anaesthesia and Apnoea Neonatorum after Caesarean Section

SIR,—Dr. J. G. Bourne (April 28, p. 984), in describing a technique of anaesthesia for obstetrical patients that is under clinical trial at St. Thomas's Hospital, does not mention what appears to be the greatest potential danger of all. This, strangely enough, is hypoxia, which will rapidly occur when intubation is difficult and the passage of the endotracheal tube and eventual inflation of the patient delayed. The inhalation of a 50% mixture of cyclopropane and oxygen from a 6-litre bag for several breaths prior to the injection of suxamethonium leaves only a short period of time for intubation before the oxygen saturation of the obstetrical patient begins to fall. An inexperienced or slow anaesthetist will find this time far too short, and even the expert will need to make haste for complete safety.

If the patient inhales 100% oxygen for a minute or two before the induction of anaesthesia, safety is increased, but manual inflation of the lungs after paralysis and before intubation is not advisable, since it may lead to reflux from the stomach or oesophagus. Intubation is made more difficult by placing the patient on her side.

This method of anaesthesia is effective in hospital obstetric practice, but should only be used by competent and experienced anaesthetists. Unfortunately it is all too often the junior residents who are left to deal with these difficult cases.—We are, etc.,

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SIR,—We have read Dr. C. A. G. Armstrong's letter (*Journal*, April 14, p. 860) with interest, and in particular his comment that "caesarean section should be classed with intestinal obstruction . . . as an operation in which the danger of vomiting is so grave that a cuffed endotracheal tube should be a *sine qua non*." He has also stated that forceps delivery might qualify for this type of anaesthetic. With both these suggestions we would like to express agreement. To us it seems that both procedures present exactly the same anaesthetic hazard—namely, the regurgitation of stomach contents. For too long gas, oxygen, ether induction with its frequently inevitable straining, swallowing, vomiting sequence has been advocated by the traditionalists as the correct anaesthetic for these cases. With the acceptance of thiopentone–succinylcholine–endotracheal-tube induction, the nightmare of anaesthetizing cases of intestinal obstruction has ceased to exist. Why not apply the same principles to all obstetric cases? In our opinion, the only legitimate reason for not doing so would be increased risk to the foetus. One might imagine that the anoxic episodes so frequently associated with gas, oxygen, ether induction could well be the cause of more harmful effects in the foetus than the use of a small, single dose of thiopentone. In an attempt to ascertain whether this supposition is in fact true, we are at present keeping records of all obstetric cases in relation to the type of anaesthetic they receive, and to date have used the thiopentone–succinylcholine technique with, at any rate, superficial success.—We are, etc.,

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SIR,—Dr. C. A. G. Armstrong's letter (*Journal*, April 14, p. 860) relating to anaesthesia for caesarean section raises several interesting points.

First, had he been able to be present at the recent joint meeting of obstetricians and anaesthetists, convened at the Royal Society of Medicine to discuss this very topic, he would have realized that he was crying for the moon in seeking "authoritative pronouncements on the proper modern methods of anaesthesia for this common operation." It was obvious that the anaesthetists had no golden rule of safety, while the obstetricians made it abundantly plain that it was high time our great specialty devoted some serious thought to the solution of this problem.

Despite the lack of skilled guidance, Dr. Armstrong has surely arrived at a reasonably safe technique, its great advantage for the occasional "caesarean anaesthetist" being that it is the one he uses for any major surgical operation to-day, and therefore one with which he is thoroughly familiar. Two criticisms of the technique would seem justifiable. Induction is surely safer if the patient is already in the head-down position. Time and vomit wait for no man, and the bronchial tree fills with fluid unheralded and unsung, which automatically demands that a bronchoscope be routinely at the ready as well as all the suction paraphernalia. In every chest unit these two items are as much a part of the anaesthetist's equipment as his laryngoscope and endotracheal tubes, yet they are rarely likely to be life-saving as they can be when there is a risk of vomiting. One day, it is not too much to speculate, they will be on every anaesthetic trolley in the country, and every anaesthetist will be also an expert bronchoscopist.—I am, etc.,

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Trendelenburg Operation

SIR,—Assuming that the medial border of the adductor magnus is taken as the medial wall of the femoral triangle, I was a little disheartened to see that Mr. P. G. Bevan, Dr. S. H. Green, and Professor F. A. R. Stammers's case 1 (1952) (*Journal*, March 17, p. 610) went to operation without the low termination of the internal saphenous vein being recognized. Anatomically and surgically it is a long way from the expected site of the fossa ovalis down to the apex of Scarpa's triangle, and I would have hoped that a careful examination of the leg before operation would have disclosed this significant fact. Although I was glad to see that by case 3 (1955) this matter had been rectified, the Birmingham paper still leaves me very worried in my mind and only tends to confirm the clinical impression that one inevitably gathers at the receiving end of a large specialist clinic.

Indeed, I have no hesitation in stating that the so-called Trendelenburg operation is not only "one of the most commonly performed in surgery," it is the *worst* performed. It is an operation that I should hate to have carried out on myself by anyone who had done less than 300 cases: and this perhaps sums up in my mind my main caveat against the Birmingham paper; for, while Professor Stammers warns against this one occasional abnormality, I would warn against *every* case. The more groin dissections I perform the more I realize that for all practical purposes the "normal" does not exist. In five cases on a list each groin may be different, and certainly, as Professor Stammers points out, the left and right are not even mirror image in the same patient. The "three tributaries," or, as Professor Stammers calls them, "the expected tributaries," are found more often in the textbooks than in the patient. There may be one, or there may be fifteen. But then, provided a true juxtafemoral ligation is carried out (assuming of course that the saphenofemoral junction is incompetent) the number or position of the tributaries is of no importance.

There is, Sir, no such thing as a standard operation for varicose veins, as Professor Stammers would wish us to believe. Every case must be judged upon its own particular demerits, and a careful examination must be carried out to determine the sites of superficial/deep incompetence which can then be dealt with as a planned operative procedure. It is here too that I must join issue with Mr. R. Rowden Foote (*Journal*, April 14, p. 858), for I consider the "stripping" operation to be just one more example of a routine blunderbuss procedure. Even at the Mayo Clinic the original simple "strip" has been abandoned and it is now combined with a superficial venous evisceration involving an additional six long linear incisions on the leg, hoping, to quote Mr. Foote, "that the more completely is the venous system removed, the longer will be the time before a further superficial system is likely to develop." Personally, I would say without hesitation that the more unnecessary removal of the venous system there is, the sooner new ugly skin dilatations will develop.